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MOVEMENTS IN CRYSTALLIZING MAGMA

FRANK F. GROUT
University of Minnesota

The study of the crystallization of silicate melts has led to many important conclusions regarding igneous magmas, several of which are stated in papers recently issued from the Geophysical Laboratory with the authority of actual experience behind them. Where facts have been discovered, however, concerning a particular hypothesis there has been a tendency to minimize the probability of some other quite independent hypotheses and to carry the conclusions along into regions not covered by experimental data. In several papers since the publication of Bowen's important outline of the evolution of igneous rocks[1] the present writer has endeavored to reopen consideration of some hypotheses lightly brushed aside in that paper. The arguments have brought forth some new light, but much still remains to be done.

Liquid magma is known to move as it is intruded, and by convection. Crystallizing magma (to accept a usage that seems to be approved) is also known to be moved by intrusion and convection, and there are molecular movements by diffusion; in addition there are probably differential movements as crystals settle or float out of magma, and there may possibly be a straining or filter-pressing away from crystals. The magnitude of diffusion is pretty definitely settled by Bowen.[2] Little question has arisen over the movements of intrusion, but there is still question as to (1) the activity of convection at depth, (2) the effectiveness of crystal settling, (3) the mechanics and effects of filter-pressing.

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[1] N. L. Bowen, "The Later Stages of the Evolution of Igneous Rocks," *Jour. Geol.*, XXIII, Supplement, November-December, 1915.

[2] N. L. Bowen, "Diffusion in Silicate Melts," *Bull. Geol. Soc. Amer.*, XXVII, p. 48.

CONVECTION

In making an estimate of the rapidity of convection[1] an argument was built up by starting with a variety of assumptions. This was done because several writers had assumed, without any suggestion of calculation, that the magma was too viscous to circulate. The value of assumptions not based on quantitative data may be illustrated by references to the effects of viscosity. Harker approves diffusion, but considers the magma too viscous to allow settling of crystals. Daly approves settling, but considers the magma too viscous to allow convection. Pirsson approves convection, but considers the magma too viscous to allow diffusion. Thus we get around the circle and all processes are still worthy of consideration.

The results of calculation, in actual figures, indicate great probability of some circulation. The calculated rates are not to be taken as at all accurate. Neither is it important to distinguish whether cooling, gas phase, or crystal phase is the dominant cause.[2] The evidences are sufficient to indicate some active circulation. Bowen says now that he has never doubted the reality of convection in magmas.[3]

The contrast presented in a paragraph headed "Crystal Settling vs. Convection" is apparently misleading. The present writer had no thought of differentiation except through the agency of crystallization. Convection seemed a necessary factor in explaining the combination of banded and fluxion structures. It is argued that the rhythm of settling would give the same banding as rhythm in currents. To this there is no question at all. But, in addition to banding, the Duluth rocks show an *orientation*. The combination is explained only by circulation.

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[1] Frank F. Grout, "Two-Phase Convection in Igneous Magmas," *Jour. Geol.*, XXVI (1918), pp. 481–89.

[2] Bowen in a later paper questions whether the two-phase idea can be extended to aggregates of crystal and liquid. Let him try it. Simply suspend crystals in side of dish and release. His own reference to heavy liquid separations indicates that he has had experience with an operation in which two-phase convection is very common.

[3] N. L. Bowen, "Crystallization-Differentiation in Igneous Magmas," *Jour. Geol.*, XXVII, p. 412. One would certainly infer a doubt from the statement in the earlier paper, p. 12: "The same objections apply to the supposed maintenance of approximate uniformity . . . . through the agency of convection currents. . . . ." Certainly convection would *tend* to keep the composition uniform, and one would infer that since the magma does not remain uniform he saw no evidence of convection.

Convection, as a general stirring, may be supposed to interfere with crystal settling. Probably in one sense this is true, for any liquid containing sediments clears more rapidly if left quiet. But the arguments for a circulation at Duluth are conclusive and have not been answered. Settling, if it occurred at all (and it probably did), occurred during circulation. Since the calculated rates of motion indicated that convection was more rapid than settling,[1] it is clear that convection might aid in bringing a crystal formed at the top of a magma chamber to some point near the bottom. Then as the current moved along the floor there might be settling enough to cause the growth of a rock layer at the bottom. Settling, however, is not the only process by which rock may grow. Simple cooling makes the shell of solidified wall or floor increase in thickness. The bottom layer of the moving current may become part of the floor by cooling and increasing in viscosity as well as by settling. Thus it may happen that even when circulation tends to prevent settling some settling may occur, and certainly some rock may accumulate along the floor.

In order to "make a case" for convection certain complications in the process were omitted in the original paper.[2] No doubt convection is far from a simple motion in large sheetlike chambers. There are the irregularities in the banding of most banded igneous rocks to prove the complexity of motion. There may even have been an approach to the theoretical hexagonal cells[3] of circulation. These, however, would have little effect on the result. No crystals would deposit on the sides of a cell—there is no support on the walls of such cells. Deposition would occur in bands on the bottom, almost as if the circulation was the simple case described.

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[1] Frank F. Grout, *op. cit.*, p. 494.

[2] Unfortunately an error occurs in the copy of the calculation for gas-phase convection, *op. cit.*, p. 491: "lbs. per sq. inch" should be *atmospheres*. It seems correct values were used in calculation, but stated wrongly in copying.

[3] See *Jour. Geol.*, XXIV, pp. 219 ff.

CRYSTAL SETTLING

The reality of the separation of crystals from liquid magma by gravity has been clearly shown.[1] In crucibles a few centimeters deep, crystals formed and settled and grew to considerable size.

In order to determine equilibrium relations, efforts were made in the laboratory work to maintain uniform conditions of heat through the whole melt. Contrasted with these conditions are the large size and irregular cooling of intruded magmas. A temperature variation of 100° C. is not unlikely in the viscous liquid beginning to crystallize. If crystals form in a cooling top layer and settle to a hot interior layer they remelt and make the magma heterogeneous. Eventually the continued settling of crystals may so cool the central part of the magma that the crystals formed near the top settle through the central part without either growth or solution. It would take so long for this balance to be attained, however, that it seems likely that the roof phase would grow as a solid before many crystals settled. Meanwhile in any large mass the cooling sides of the chamber would start convection and entirely modify the situation. The probability seems to be that chambers a few feet thick are chilled; up to 100 feet crystal settling may be the dominant effect, but in larger chambers convection stirs things up too much. Settling a few feet removes crystals from the circulating magma.

The condition of the gabbro at Duluth was taken as a sign that no great amount of settling occurred,[2] and Bowen agrees that simple settling does not adequately explain the series of rocks developed. He finds it possible still to refer to the peridotites as a result of settling. They are bands, like other bands in the gabbro, and if most of the bands are not the result of settling it seems inadvisable to select the peridotites to support the original idea.

In a remark on the banded structure at Duluth, Bowen says that if crystallization is rhythmic, crystals brought down to the bottom by settling would result in precisely the same alternation as if brought down by convection. In this he is wholly correct and no advantage is claimed for convection. It is only when the banding is accompanied by an orientation of grain that the evidence is strong against crystal settling. It is not the banding nor the parallelism which determines the process, but the combination of bands with a foliated structure. This was emphasized in the original papers.[1] As there stated, crystal settling is to be thought of as a matter of a few feet, not thousands of feet.

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[1] N. L. Bowen, "Crystallization Differentiation in Silicate Melts," *Amer. Jour. Sci.*, XXXIX (1915), p. 186.

[1] Frank F. Grout, "A Type of Igneous Differentiation," *Jour. Geol.*, XXVI (1918), p. 639.

FILTER-PRESSING

A process of expulsion of residual fluid magma from a mass after a large proportion of the material had crystallized is advocated by Bowen and Harker. Preliminary suggestions of the idea are credited by Harker to Barrow and to Judd (who refer also to Osann, Teall, and Geikie).

In tracing these earlier statements of the idea, no good outline of the mechanics or general results could be found. Teall and Osann and Harker[2] describe patches of glassy or residual magma filling steam cavities when the gas of the cavity has been absorbed or condensed, or has escaped. No one can question the occurrence of rounded lumps formed from magma, but the source of the lumps and the process by which they got there, and the ultimate removal of any gas that may have been there, are hard to understand. If the magma was liquid enough to "ooze out from among the crystals" the gas just separated from the magma could hardly be absorbed, condensed, or allowed to escape, leaving a spherical hole. Barrow[3] has described a pegmatitic separation from granite, as have many others. In many cases this may be a process of separation of liquid from partly crystalline magma, but pegmatites are most readily explained by the readier penetration of thinly fluid *emanations* in advance of viscous magma. This is more nearly analogous to immiscible separation than to filter-pressing. Filter-pressing involves differential pressures, and no such pressures are evidenced by the great majority of granites having pegmatitic facies. Barrow also refers to a case of straining off of magma into crèvices too small for crystals to get in. Such an action is easily understood but would not be expected to produce large bodies of rock. Barrow goes on to say, however, that "the continuance of the pressure will still further force the liquid from the solid crystals, leaving at last just sufficient of the magma to fill the interstices between them." This can hardly be considered a clear outline of what happens "further" than the oozing of magma into cracks too small for the crystals to enter. Judd[1] has described andesite and pitchstone intrusions which he thinks separated by a process of "liquation"—apparently a growth of crystals along the walls leaving a residue of different composition. Harker, referring to the suggestion, says "it is easy to conceive of various modes of liquation and decantation, straining and filtration by which a partial separation may be brought about." Bowen has shown the improbability of diffusion sufficient to yield much differentiation by any liquation in this sense. "Decantation" is not a process of filter-pressing. To the present writer "straining and filtration" of a magma on any large scale are not easily conceived.

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[1] Frank F. Grout, "Internal Structures of Igneous Rocks," *Jour. Geol.*, XXVI (1918), p. 455.

[2] A. Harker, *The Natural History of Igneous Rocks*, pp. 324–25. Macmillan, 1909.

[3] George Barrow, "On Certain Gneisses and Their Relation to Pegmatites," *Geol. Mag.*, 1892, pp. 64–65.

Harker reviews these descriptions, emphasizing the case of pegmatites and that of pitchstones related to andesites. He does not refer to any indication of a process of compression except in the case of numerous small pegmatite intrusions related to large granite gneisses. Even here the larger masses of pegmatite are better explained by a separation of some other sort.

Bowen presents the latest development of ideas on filter-pressing.[2] His first analogy is to the squeezing out of water from sand on a wet beach. There is little question that such a mesh of crystals as he suggests, up to 80 per cent of the total mass, might, when subject to differential pressure, give way and result in closer packing.

The supposed filter effects are described for three different cases, but no mention is made of the case of pegmatitic separation suggested by Barrow. First, horizontal compression of a laccolith, crystallized to about 80 per cent, might yield an upper differentiate of liquid;[1] the effect on the crystallizing residue is clearly described as follows: "The horizontal dimensions are shortened in consequence of closer packing of crystals." Second, horizontal compression of a sheet in which the top and bottom had partly crystallized might result in a squeezing of the liquid into the central zone; here again the crystalline residue will have its horizontal dimensions shortened. Third, there may be a warping of the walls of a sheet; the forces which are expected to compress part of the sheet to such a degree that other parts are thickened are not described. It would seem that there is no reason to assume a *central* accumulation of the liquid in this case, any more than in the case of a laccolith, where an *upper* layer is supposed to accumulate. Both sheets and laccoliths have competent roofs. Both are likely to have their upper layers solidified early.

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[1] J. W. Judd, *Quart. Jour. Geol. Soc.*, XLVI (1890), p. 379.

[2] N. L. Bowen, "Crystallization-Differentiation in Igneous Magmas," *Jour. Geol.*, XXVII (1919), p. 393.

There are thus outlined three cases in which Bowen finds no mechanical difficulty in imagining an action like a filter press. Two fundamental objections may be raised and should be answered before final acceptance of the idea. The first has to do with the structures left in the crystalline mesh after expulsion of the fluid; the second is a matter of the completeness of the separation, and the volumes of the separated parts.

Structures.—These three types of filter-press action occur when the mass is 80 per cent or more crystalline. Such a mass would have the crystals pretty well locked together. Uniform spheres closely packed take up only about 75 per cent of the space in which they are packed. Packed angular grains may occupy as little as 50 per cent. Even 5 per cent further growth would result in a fairly firm bond between adjacent crystals. If now the mesh was deformed, the crystals would be broken and bent in rather violent fashion. Rock flowage would be evident in the examination of the grains. It is possible, of course, that the temperature is so high and conditions so favorable for recrystallization that some of the strain effects would later disappear, but the broken crystals of porphyries and such rocks as the Sudbury norite argue against such effects. Even if recrystallization occurred, the process would be accomplished under stress and an orientation of grain would be almost inevitable. It is worth while, then, to investigate the cases of supposed filter-pressing for signs of structure and deformation. It would be a strong confirmation of Bowen's argument if a laccolith or sill could be cited in which the fluxion structure corresponded to the position of such differentiates as he describes. Neither Bowen, Judd, Barrow, nor Harker makes any mention of such an orientation. On the contrary, in the illustrations of the glass supposed to have oozed into vesicles Judd and Harker show exactly the opposite orientation. It looks much more as if a lump of solid glass had interfered with the haphazard position of the crystals.

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[1] Bowen carries the idea too far when he attempts to apply it to the Duluth lopolith. Lateral thrust on a sunken basin would hardly tend to dome up the roof. Furthermore he makes an inaccurate copy of a map published with a plain statement that the area on which he bases his argument had not been mapped in detail.

EDITORIAL NOTE: In this connection it may be said that the map which Dr. Bowen states that he copied is the map printed as Fig. 6 in the *Journal of Geology*, Vol. XXVI, p. 446. The map which Dr. Grout apparently has in mind in this connection is the map reproduced as Fig. 1, Vol. XXVI, of the same *Journal*, p. 628. The differences between the two are very slight.

Turning to the special case of Duluth, Bowen takes up a final suggestion of the mechanics of filter-press action by proposing a theory to account for the rocks described.[1] The great lopolith shows a sunken structure at present, and on that basis it may be assumed that there were some movements during crystallization.[2] Bowen believes that movements when the mass is 50–65 per cent crystalline would tend to produce bands and layers by the bridging action of the feebly interlocked crystal mesh. The reality of the bridging effect in a crystal mesh need not be questioned, but a more detailed report of the experimental work which led him to the suggestion and to the estimate of 50–65 per cent of crystals would be welcome. He has previously[3] stated an opinion that a magma with 50 per cent crystals was eruptible, and the bridging effect was ignored.

[1] *Jour. Geol.*, XXVII, p. 411.

[2] The main subsidence must have occurred before crystallization, for the mass is too thick to be supported isostatically as a dome. It is not improbable, however, that movements continued.

[3] *Jour. Geol.*, Supplement, November-December, 1915, p. 31.

The effect of the bridging tendency in such a crystallizing magma is stated in Bowen's words; the liquid must flow "into the space immediately below it, if the potential bridge is to become a reality." It is a most significant point that the motion is at right angles to the bands, each crystallizing layer contributing liquid to form a layer just below or just above. Furthermore, the deformation produced in the "weak yielding segment" is from lateral compression. Both the flow and the deformation would produce a *vertical* orientation. As a matter of fact the bands in the Duluth gabbro show many instances of orientation parallel to the bands and not one at right angles. It should be noted also that any filter-press action at Duluth must have stopped before much interlocking of crystals occurred, for there is very little sign of broken or bent crystals—three or four grains in hundreds of sections. There are enough to show that signs of strain may be preserved, but are so few that no general deformation is likely. Here, as in the previous cases that Bowen outlines, the evidence of structure is emphatically opposed to the action he suggests.

Completeness of separation and volumes of the separates.—If we assume, as seems probable, that liquid may be squeezed out from a crystal mesh 50–80 per cent crystalline, how complete a separation can be effected? In the banded portion of the Duluth gabbro the main rocks are olivine gabbro with only slight changes in proportion of minerals, but the bands of extreme composition are just as truly bands as the rest, and are evidently formed in the same way. Can an anorthosite be squeezed out of the same olivine gabbro as a peridotite? Or must we assume that the peridotite was the *residue* after the average gabbro liquid was squeezed out? If we assume that peridotite is a residue, can we assume for a neighboring band that anorthosite was the residue after the same average gabbro was squeezed out?

Bowen notes that the contrast between bands should be of a different order of magnitude from that shown in the gabbro-granophyr association, implying that the variation in the bands is relatively slight. The bands vary from rocks 98 per cent plagioclase to rocks 90 per cent magnetite, and include peridotites with no feldspar and troctolites with no augite. The extremes are 100 per cent different, and are not to be expected from filter-pressing.

Taking next the case of granite separated from gabbro, we find it a general rule that the separation has proceeded to such a degree that the granophyr diabase is very small in bulk.[1] Such action develops only after the magma is 80 per cent crystals. The occurrence of 300 feet of red rock above gabbro with practically no granophyr, as described at Duluth, should mean that over 1,200 feet of gabbro was almost completely drained of all 20 per cent residual fluid—so completely that the trifling residue left no determinable mineral and produced no zoning of the feldspars. Such thorough drainage from so large a mass seems very unlikely. And if the 80 per cent crystal mesh had any strength at all it must have refused to yield so completely as to leave no interstitial magma at all. Interstitial granophyr would be much more likely to be trapped among crystals than as blebs in an immiscible liquid.

SUMMARY

In igneous magmas the movements which are subject to some disagreement are convection, crystal settling, and the expulsion of residual magma from a partly crystalline mass. An attempt is made to remove certain misconceptions of the convection process and its effects. The process of crystal settling seems to be limited to effects which may be estimated to extend not over 100 feet. Attention is called to several difficulties in the application of the filter-pressing idea. The writer favors all three of the ideas as working hypotheses, but certain critical field relations should be sought for before accepting either in specific cases.

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[1] Frank F. Grout, "A Type of Igneous Differentiation," *Jour. Geol.*, XXVI (1918), pp. 645–57.